

In the middle strata through out-swelling toward the north of the stratosphere, which is colder toward the south, the high temperature of the south current coincides with high pressure; in the lower strata there arises on the east side of this high pressure a countercurrent from the north which shifts the temperature maximum to the west and the pressure maximum to the east, and thus brings about the phase difference between pressure and temperature distribution. On the other hand, the high cyclone arises from a north current, cold below and warm above; aloft the lowest pressure falls under the lowest position of the stratosphere. On its front side there develops in the lower levels a warm countercurrent which shifts the pressure minimum toward the east and the temperature minimum toward the west. The "tie" between upper and lower pressure formations is fully accounted for from this. It is a matter of only one depression which has phase shiftings, locally possibly different, but it is not a matter of two depressions, an upper and a lower, as Ficker supposed. The high pressure above is characterized by the fact that it is as a whole cold, the low pressure by the fact that it is as a whole warm. The thermal explanation of the phenomena is practicable when it is borne in mind that for dynamic reasons the isobars run from west to east, that thus at the north there lies really a lesser mass above the given high level than there lies at the south. In this way the south current brings rise in pressure aloft, while the north current brings fall in pressure.

The fact that over a region with 1,000 to 2,000 kilometers radius, and up to several kilometers elevation, the air can be warm and can meanwhile move downward, the fact of the stationary anticyclone, warm below, may be explained, so far as I see, not otherwise than by a south current in the substratosphere, whose cold air sinks at the north in warmer regions and meanwhile forces down the lower, dynamically heated air. This anticyclone has thus certain similarity to a waterfall, the center of gravity of the entire mass descends despite the descent of warm

air. We have a circulation in which the energy is furnished by the cold substratospheric air of the Tropics and the sub-Tropics.

The terms "north" and "south" are used here representatively. Taken exactly it is a matter of the position of the stratosphere relative to the season; in Europe, as is evident from the above table, the oceanic west (England) stands in similar contrast to continental Russia as normally the south stands to the north.

In the building of high cyclone and anticyclone from a lower formation the cold mass of air forcing its way at the ground is evidently the bearer of high pressure until the higher air strata of the stratosphere are drawn into the northerly and southerly winds. With this, then, the highest pressure over the western limit of the cold air goes out over the warm air following (in the circulation—*Translator*), the lowest pressure removes from the warm southerly current westward into the mass that is cold below. The cold wave precedes the high pressure in the advance toward the east. We are not able to judge as to what extent the inward and outward curvatures of the stratosphere change independently.

BIBLIOGRAPHY.

- (1) Die Beziehungen zwischen Druck und Temperatur in der freien Atmosphäre. *Beitr. z. Phys. d. fr. Atm.*, Bd. 9, Heft. 4.
- (2) I owe the values t_0 to courteous communication from the author.
- (3) *Meteorol. Zeitschr.* 1921, S. 65.
- (4) *Sitzungsber. d. Akad. Wiss.*, Wien, Bd. 119, Abt. 2 a, S. 697, 1910.
- (5) Compare my paper in *Meteorol. Zeitschr.* 1913, S. 434.

TRANSLATOR'S NOTE.—The sentence "Gehen wir zur Obersten Troposphären schicht herab, p. 297, line 15, is rendered with interchange of the words "warme" and "kalte" since this appears to be in agreement with the curves p_h and t_h .

ORIGINAL TEXT.—Über den Aufbau hoher Antizyklonen und Zyklonen in Europa. Felix M. Exner. *Meteorologische Zeitschrift*, 1921, Heft 10, Oktober.—W. W. R.

THE TORONTO SYMPOSIUM ON BAROMETRIC REDUCTIONS.

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A suggestion made at the Chicago meeting of the American Meteorological Society in 1920 developed, at Toronto, into a satisfactory symposium on the reduction of barometric pressure in the United States and Canada. This symposium should not be regarded as a culmination of effort, but rather as a significant step toward a goal, the road to which is beset with many pitfalls and obstacles. But there is little doubt, it is believed, that this symposium does signify much for the future of American barometry. For 20 years, the United States, Canada, and the West Indies have used the system of barometry devised by Bigelow. This system was, without question, a thorough and capable treatment of one of the knottiest problems of practical meteorology; but it was not exhaustive—even in the mind of Bigelow the work was not finished—revisions and reexaminations of the gradients, methods, and errors, should follow after a few years of taking homogeneous observations. But these 20 years have passed. Those most familiar with the reductions are well aware of the fact that they are not the best; at the same time, improvement of Bigelow's treatment is regarded as most difficult. The Toronto symposium was born of this consciousness of the difficulties of reduction. Whether this symposium will result

finally in an improved system of reductions depends upon how thoroughly and searchingly the problem is studied, and how practical and workable are the suggestions offered by investigators.

In order that those who did not have the opportunity of attending the Toronto meeting may be informed of this interesting session, it is believed that a brief résumé of the several papers, in the order of their presentation, may be of value.

The nature of the problem from the practical standpoint was stated in a short communication from Prof. C. F. Marvin, Chief of the United States Weather Bureau. He said, in part:

It is well known that, strictly speaking, a reduction to sea-level of continental observations is wholly a visionary and hypothetical thing, since to effect the reduction we must necessarily assume that an atmosphere exists below the station whose observations are to be reduced. We assign to this atmosphere a given temperature and moisture content, density, etc., and our reduction depends upon these assumptions. Since the intervening atmosphere has no existence our assumptions are necessarily fictions, and the final result of the so-called reduction to sea-level has in reality no meaning.

He then explained how, by so treating the temperature argument and the other assumptions that they will satisfy the requirements for smooth isobars on the daily

weather map, the forecaster is enabled to forecast weather even upon the basis of appearances on this assumed sea-level. Attempts to remedy these difficulties, which are especially acute in the plateau regions of the west, have led to the suggestion that maps of free-air levels would be a possible panacea. But such maps, feared Prof. Marvin, would require the training of a new school of forecasters, owing to the fact that upper-air maps would bear a different relation to surface weather from that of the present sea-level maps. He concluded that:

It appears very problematical if a change in present practices of reducing barometric pressures as represented by the reduction tables used by the Weather Bureau, or others that are appreciably equivalent thereto, will prove of value. However, the academic study and examination of the problem is one of the very greatest interest and should be pursued in an effort to minimize what appear to be the unavoidable errors and fallacies involved in present systems.

The last sentence of Prof. Marvin's remarks, it seems to the writer, constituted a very acceptable keynote for the symposium. Moreover, the burden of his remarks indicates that, in the academic study of the question, the element of practicality must not be forgotten. While it is easy and often desirable for the scientific investigator to disencumber himself of the practical aspects of the problem, it seems (and the history of American barometry proves that this is true) that in a study of this character the investigator must constantly project his studies against a background of practicality. One must take care that one's suggestions are practical, else the labor expended on the problem may bear no greater fruit than a spreading of printer's ink.

The writer contributed two papers to the discussion, the first of historical character, and the second suggesting the possibilities of upper-air maps. The first paper entitled "The history of barometry in the United States," covered the salient features of each of the several methods of pressure reduction that have been employed in the United States since the beginning of the Signal Service, namely, the Guyot tables, the Abbe-Upton monthly constants, the Ferrel tables, the Hazen reductions, and the Bigelow tables at present in use. These various methods are too complex for hasty review here, but it is hoped that the extended paper may be published within a few months. From the several methods, attention was drawn to three features that were common to them all: (1) All methods reduce to sea level; is this the most suitable level? (2) All methods have their weakness in the assumption of a hypothetical atmosphere below the station and in the treatment of its temperature; is Bigelow's treatment of the temperature argument, which is admittedly complex, the most satisfactory, and might we not improve upon it in sea-level reductions after acquiring so many years of homogeneous records, and much aerological information that Bigelow did not have? (3) All workers found peculiar pressure discrepancies, left unexplained by Bigelow, which he said should be given further study; are these discrepancies real, or will not a reexamination of station records possibly either eliminate them, or lead us to their cause and an understanding of their nature? Heretofore, they have been buried or concealed behind corrections, but never explained. From these considerations, it appears that, regardless of the advisability of introducing other reduction levels than sea level, a new study of pressure data and a reexamination of them would probably lead to better reductions to sea level.

The second paper discussed the results of continued research upon the possibilities of drawing accurate pressure maps at the levels of 1 and 2 kilometers above sea level. The central assumption or keystone of this work is that surface-wind directions—assuming, of course, an open and free anemometer exposure—are reliable indices of conditions of temperature, hence pressure, aloft. A study of all available kite data in eastern United States abundantly justifies this assumption, and it is confirmed in recent statistical data summarized by the Aerological Division of the Weather Bureau showing the average seasonal deviations of wind with altitude from surface direction and the percentage frequency of clockwise and counterclockwise turning.

Two papers were read by title, owing to the absence of the authors, the first by Prof. Alexander McAdie, and the second by Mr. W. G. Reed. The first on "Sea level *vs.* the megadyne base," recommended the practice of reducing to standard bases, differing according to station elevation. The author said: "* * * thus, if a station is approximately 100 meters above sea level, the base is 1,000 kilobars; if the elevation is 1,000 meters, the base would be 900 kilobars; at 10,000 meters, 300 kilobars." The second on "Major wind streams *vs.* high-and-low-pressure centers as the basis for weather forecasting" contained matter of interest, part of which was touched on in the discussion. Mr. Reed contended, and probably rightly, that the true sources of our weather changes are the interrelations of the great streams of air, each with its peculiar and characteristic temperature, moisture, and rate of movement. He believes that our scheme of following day to day pressure changes is only an indirect method of getting at those streams of moving air. He said:

It is my belief that we are really concerned with moving air and the systems of air streams. If this is the case, we should map air movement rather than air pressure. Our maps should show the positions, directions of flow, and speeds of air streams, together with any changes in direction and speeds. These figures, rather than pressures, may be the most important facts to be presented as showing cyclonic conditions. I realize, of course, that we have built up in the past half century a system of handling air pressures which may readily be mapped, and which possibly may give us indirectly the same information as we should get directly by mapping air streams. Furthermore, the technique of mapping air movement is as yet only partly developed.

However, I believe the place of air pressure in meteorology should be reconsidered. This element doubtless has a historical value and is of great importance in empirical forecasting; its place in a rational system of forecasting based upon the physics and dynamics of the atmosphere is not so clear. Undoubtedly pressure is easier to observe than air movement, especially upper air movement in cloudy weather. For this reason, at least it is probable that the observation of pressure should be continued to provide a foundation for interpolation to supply gaps in data for air movement obtained by direct observation, if for no other reason.

Thus, it appears that Mr. Reed in preparing his paper, realized what Maj. W. R. Blair, in a written discussion said, namely, that he doubts whether "weather services will be able to come to it in practice, even though they appreciate its validity." It might be suggested that the element of practicality, mentioned earlier in this discussion, is the pitfall toward which this suggestion leads; it is undoubtedly true that an adequately developed method for charting the great air streams would be of untold value, but how can it be done? No one can say that future development may not be in that direction; but, at present, with our loose-meshed network of aerological stations, it can not be seen wherein this offers a helping hand to the limping plateau reductions.

Mr. Reed next brought up the question of departures from station normal as a possible substitute for sea-level reduction. This question was also raised in the discussion by Dr. J. Patterson, who said that in India, the practice of sea-level reduction applied only to stations of less than 3,000 feet altitude, and that above that level, departures from station normal were used with success. Dr. Humphreys said that while this suggestion was so old that it was difficult to say who first proposed it, so far as he knew, no thorough examination of its merits or faults has been made, and he suggested that such an examination be made. Dr. C. F. Brooks urged the criticism that horizontal differences of normal pressure might introduce the conception of erroneous gradients when departures are plotted. He cited the case of the Azores HIGH and the Iceland LOW and showed how a negative departure of pressure at the Azores and a positive departure at Iceland might obtain and still the Azores pressure be higher than that at Iceland. Of course, this was an extreme case and, owing to lesser geographical distances, might lose some of its force. He showed, however, that maps of departures from normal could be used in connection with maps of normal distribution and departures from the normal gradient winds computed. These computed departure of winds would be of significance relative to departures in temperature. Thus, a wind which was more northerly than usual would in the United States, ordinarily indicate the existence of, or the tendency toward, a negative departure in temperature. Another consideration concerning this question was that of cases where stations of considerably different altitude might be quite close together. It is known that when on account of cold weather the pressure is high at the surface a LOW is found aloft; this might cause positive departures at the lower station with negative departures at the upper station, an altitude effect entirely. This might introduce difficulties where the plateau and Great Plains meet. Dr. Patterson admitted the validity of these considerations, but said that his experience in India had not indicated that such difficulties were serious. At any rate, an investigation of this problem would be most attractive and offer opportunity for some good work.

Prof. H. J. Cox, of the Chicago office of the Weather Bureau, stated that, from the forecasters' point of view, some improvement in plateau barometry would be eminently desirable. He cited the situation that obtains when a HIGH is situated in the northern plateau and a LOW to the southwest. The northeast-southwest gradient as shown on the sea-level map is then so steep that if the winds were blowing with a force indicated by the close-

ness of the isobars "they would blow the mountains away." Such gradients are nothing if not misleading. The forecaster may be able to make proper allowance for them, but the student, as Dr. Humphreys pointed out, is helpless and confused when confronted by such a situation. Assuming that such a gradient would obtain at sea-level if the continent were removed, we have yet to inquire of *what value is this fact in actual forecasting?*

The symposium was closed by a paper by Dr. Brooks on the use of clouds in local forecasting, explaining how satisfactory temperature forecasts for Worcester, Mass., had been made on the basis of upper-air maps, deduced from cloud movements.

In order that the substance of the several papers and the opinions expressed in the discussion might be given some tangible form, a resolution was drawn up and adopted at the afternoon meeting of the following day. The unofficial character of the symposium and the views expressed were recognized in the resolution. Its purpose was rather to stimulate the several weather services concerned to investigational activity than dogmatically to express an opinion. This resolution follows:

Whereas the reduction of barometric pressure to sea level, as practiced in the United States and Canada, is generally conceded to be unsatisfactory under extreme weather conditions, especially in the plateau region of the west; and,

Whereas the sea-level reductions afford an unsatisfactory basis for the forecasting of upper winds for the benefit of aviation; and,

Whereas a careful study of the method of reductions at present employed, devised by Bigelow 20 years ago, reveals that he recognized the necessity for a reexamination of barometric data after such time as all stations might have long homogeneous records; and,

Whereas all students of the barometry problem, including Bigelow, have encountered anomalies or discrepancies at certain stations, manifested by the failure of such stations to conform to the distribution of pressure in their vicinity, and since no satisfactory scientific explanation of the irregularities has been advanced: Therefore be it

Resolved by the American Meteorological Society, assembled at Toronto, Canada, this 29th day of December, 1921:

1. That the time is now opportune for a reexamination of the barometric methods employed in the United States and Canada with a view to the possible improvements of pressure reductions.

2. That such investigations should include an examination of—

(a) The accuracy of maps of the free-air, and their value as aids to forecasting for aviation and general forecasting of weather.

(b) The value and practicability of construction daily charts of the departure of station pressure from normal as aids to forecasting in the plateau region of the west.

(c) The validity of Bigelow's temperature argument which is included in station reduction tables at present in use.

(d) The nature and cause of the barometric discrepancies noted above and their true values.

3. That a copy of these resolutions be forwarded to the chiefs of the Weather Services of the United States, Canada, and Mexico, inviting their attention to the considerations herein, and urging that they give these matters their earnest consideration at the earliest practicable time.